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By

MARY C. VAN TUYL

A dissertation submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in the University of Michigan.

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Studies in monocular perception of distance have centered about the relation of accommodation, or of accommodation and convergence, to the estimation of the third dimension, and have usually been narrowed to the estimation of relative rather than absolute depth. Baird summarized the investigations up to 1903 when he added his contribution to the subject.¹ All of these investigations, with the exception of Bourdon² and including Baird, were positive in their results: that is, it was unmistakably possible for one eye under the different experimental conditions and within limits to perceive the relative distance of the experimental objects. The interpretation of the results differed, however; all except Hillebrand,³ and Dixon,⁴ insisting that accommodation, or accommodation and convergence, were definite muscular factors responsible for the estimation of distance. Hillebrand went further than the muscular movement for his basis and described, as the effective factor, a "conscious impulse of will innervating the accurate adjustment of accommodation." Dixon verified Hillebrand's results, on Hillebrand's apparatus, but disagreed with his conclusions, saying, "It would seem that the actual criterion was . . . a difference in the rapidity or ease with which the accommodation adjusted itself (or

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¹ J. W. Baird, The influence of accommodation and convergence upon the perception of depth, this JOURNAL, 14, 1903, 150-200.

² B. Bourdon, La perception monoculaire de la profondeur, *Rev. Phil.*, 46, 1898, 124-145.

³ F. Hillebrand, Das Verhältnis von Akkommodation und Konvergenz zur Tiefenlokalization, *Zsch. f. Psychol.*, 7, 1894, 97-151.

⁴ E. T. Dixon, On the relation of accommodation and convergence to our sense of depth, *Mind*, 4, 1895, 195-212.

was adjusted by the observer) and not in any conscious direction of accommodation by the observer."

Nowhere in English is there mention of the investigations of Giering,⁵ Ascher,⁶ Peter,⁷ and Bappert,⁸ which came later. These investigations, like Bourdon's, were negative in their results. Therefore, practically all that is presented in English, excepting Baird's brief mention of Bourdon's work, takes for granted the definiteness of effect of accommodation, or of accommodation and convergence combined, in the monocular perception of distance, under experimental conditions which have sought to eliminate at least the influence of size and intensity in the experimental objects.

The present investigation started from these results to seek an explanation or a reconciliation of such opposition, or, if possible, with better methods and apparatus to prove the one or the other side justified in its position.

The table which is usually reproduced as indicative of Wundt's general results in respect to monocular perception of distance, with threads as fixation objects, presents definite limens of approach and recession which decrease directly with decrease in distance of the object from the eye.⁹ Nowhere is attention called to the fact that this table, the only table given by Wundt, represents one observational series only, of one subject whom Wundt describes as "having a somewhat farsighted eye, of limited range of accommodation, and whose far-point and near-point were, respectively, 250 and 40 cm. from the eye." This sort of datum was the basis of Wundt's conclusions. Wundt's work stood unchallenged for nearly four decades and still forms the basis for many of the statements which appear to the present day.

Hillebrand published, in 1894, his criticism of Wundt's procedure, results, and conclusions and proceeded after extended theoretical discussion to a different sort of attack upon the problem. Arrer published (1896) an extensive piece of work done under Wundt's direction in which he substantiated Wundt's procedure, results, etc., and repeated parts of Hillebrand's work.¹⁰ The following year Hillebrand published a lengthy review of the whole problem with renewed criticism of the other investigations and further justification of his own conclusions.¹¹

⁵ H. Giering, Das Augenmass bei Schulkindern, *Zsch. f. Psychol.*, 39, 1905, 42-83.

⁶ K. Ascher, Zur Frage nach den Einflüsse von Akkommodation und Konvergenz auf die Tiefenlokalization und die scheinbare Grosse der Sehdinge, *Zsch. f. Biol.*, 62, 1913, 508-535.

⁷ R. Peter, Untersuchungen über die Beziehung zwischen primären und secundären Faktoren der Tiefenwahrnehmung, *Arch. f. d. ges. Psychol.*, 34, 1915, 515-564.

⁸ Jakob Bappert, Neue Untersuchungen zum Problem des Verhältnisses von Akkommodation und Konvergenz zur Wahrnehmung der Tiefe, *Zsch. f. Psychol.*, 90, 1922, 167-203.

⁹ W. Wundt, Beiträge zur Theorie der Sinneswahrnehmung, III, *Zsch. f. Rat. Med.*, 3, 1859, 330.

¹⁰ M. Arrer, Über die Bedeutung der Konvergenz und Akkommodationsbewegungen für Tiefenwahrnehmungen, *Phil. Stud.*, 13, 1896.

¹¹ F. Hillebrand, In Sachen der optischen Tiefenlokalization, *Zsch. f. Psychol.*, 16, 1897.

In the face of Wundt's and Arrer's criticisms of Hillebrand's apparatus, Baird (1903) chose to use that apparatus in his attack upon the problem. So certain was he of the important character of his contribution that he says, "If our results are, as we believe they are, decisive, they furnish yet another instance of the celerity and certainty with which the experimental method can deal with disputes of long standing."¹² This work, with Dixon's, forms the main experimental contribution to the problem written in English. In this country at least, although criticized in minor respects, it is apparently undisputed in its general conclusion.

The Hillebrand apparatus, used also by Dixon and by Arrer, had consisted essentially of a pair of vertical black screens mounted upon a movable horizontal frame. This frame was pivoted upon a table and was so constructed that movement in the horizontal direction brought into view first the edge of one screen and then the edge of the other. The edge when visible was in the direct line of sight of the subject and stood out sharply against a lighted opal glass background. The field of view was therefore a circular area limited by a diaphragm in the end of an observation tube and appearing as a half-black, half-white field. The black was screen, the white was background. Successive exposures alternated the black-white relations. The sharp delimiting line of division (*i.e.* the screen edge) was designed to be the fixation-point. In his use of the Hillebrand apparatus Dixon had so constructed the frame that the mounted screens always came from one side. The relative position of black-white areas remained the same, therefore, in succeeding exposures. Baird, however, retained Hillebrand's original construction in this respect. In order to remove the objection of the influence of irradiation and contrast in the visual field in Hillebrand's apparatus, Baird reduced the illumination slightly. As to the other main objection, that a mathematical line affords no adequate fixation-object, Baird simply states that no one of his numerous visitors ever failed to see the screen edge as a clear-cut, well defined line perfectly possible of fixation.¹³ Certain refinements were added to the original Hillebrand apparatus, but these were in nonessential details.

Swenson¹⁴ and Carr¹⁵ have criticised Baird's investigation in detailed manner and justifiably, on the same grounds. They both accept, however his main conclusion in respect to the positive effect of sensations of accommodation in monocular depth-perception, and in their contributions they both continue the succession begun by Wundt. They are both, however, in error in classing Hillebrand within that succession, and in describing his approach and his conclusions in the general terms of that succession. Baird's historical summary makes Hillebrand's position perfectly clear.

Those investigations which presented essentially negative results on muscular factors need to be considered more in detail, with the possible exception of Bourdon's, whose work is well known. Bourdon described various procedures, some with points of light, others with luminous circles, etc. His results were largely negative. He explained them by other factors than accommodation and convergence. He questioned whether there is any sensation of accommodation, and whether the sensation vaguely painful in considerable accommodation may not be a sensation of con-

¹² Baird, *op. cit.*

¹³ Baird, *op. cit.*, p. 171; also see footnote ²⁷ below.

¹⁴ H. Swenson, The relative influence of accommodation and convergence in the judgment of distance, *J. Gen. Psychol.*, 7, 1932, 360-378.

¹⁵ H. Carr, *An Introduction to Space Perception*, 1935.

vergence which comes in by reason of the close association existing between the two. Moreover, he called attention to the fact, that, while convergence is very delicate in binocular vision, in monocular, for two reasons, it is only confusing: first, convergence cannot give more precise indication than accommodation, which guides it, and, secondly, convergence does not exactly follow accommodation. From two comparative experiments in monocular vision, where, in the one case, the head was allowed to move, and, in the other, it was held rigid, he decided from the positive results of the first case, that the principal aid in getting monocular depth is from this sort of monocular parallax.

Giering used 6 yr.-old and 14 yr.-old boys and girls as Ss in order to get wide and easy ranges of accommodation, and also on the ground that in adults the significance of 'muscle sensations' might have receded in importance as against other criteria. His preliminary procedure made sure that the children were able to make accurate depth-discrimination of his stimulus-objects under ordinary conditions. For the main research he used a simple apparatus which allowed simultaneous and successive exposures of vertically placed rods of different sizes. His sizes and distances were 4-, 5-, and 6-mm. diameters at, respectively, 80, 100, and 120 cm. His results are separated for age and for sex but present no significant differences. His conclusion is that, from his results at least, accommodation and convergence sensations cannot be assigned any real significance for depth-perception in monocular vision, unless it be in some cases by that indirect means which Hillebrand had described.

Ascher describes a monocular study on the influence of accommodation and convergence upon depth-localization and upon estimates of size. He used 7 Ss, in a limited number of trials. Several of the subjects were myopic, with myopia ranging from 2 D. to 6 D. All of these individuals wore correcting glasses. Three subjects only were emmetropic, while two were described as in early presbyopic condition. The stimulus objects were isosceles triangles with an opal-glass surface mounted in front of an electrically lighted lantern. The distances used ranged from 20 to 200 cm. and the diameters of the lighted surfaces ranged correspondingly from 4 to 40 mm. Ascher's conclusions were; "In these researches the apparent size depends merely upon the size of the retinal image, and differences in distance of the comparative object which require a change of accommodation up to $\frac{3}{4}$ D. are entirely without influence upon estimation of size. It may be, however, that in increased approach of the object there may be some kind of empirical cue of depth localization effective which depends upon the absolute distance of the object. What it is and in particular whether it is unavoidably involved in the procedure must in any case await further investigation."¹⁰

Rudolf Peter selected two opposing factors, the visual angle and accommodation. In part of his procedure he set these in opposition and in other parts made them supplementary and tried to obtain quantitative measures of the results. The procedure is monocular throughout, and in certain respects is more carefully controlled than any used before. Careful provision was made for rigid head-position, for a timed exposure, for exact position of object lights and for accurate size and intensity control. In his results, limited as they are to two subjects, and with insufficient numbers of trials, Peter found that the secondary factor of the visual angle

¹⁰ Ascher, *op. cit.*, 535.

prevailed over what he called the primary factor of accommodation in practically every case. The distances used ranged from 30 to 230 cm. Light boxes furnished the stimulus objects and were provided with diaphragm openings with adjustable diameters.

Bappert checked all of the previous techniques except that of Wundt's with threads, a procedure which involved too many sources of error. He eventually discarded them all, as unsuited to the problem, except a modification of the light-box used by Peter. To each of the procedures he added the use of a frame into which he could insert a lens in front of the observing eye, and of a mirror so placed that the movements of the unused eye could be observed. He used some care in excluding reflection and noise although success in the latter was questionable since the noise, *e.g.* that which occurred as a result of moving the screens of the Hillebrand apparatus, was covered by a "counter noise made by continually scraping the feet."

Certain observations which Bappert made in the use of the Hillebrand apparatus are of more importance than his data. The latter are insignificant on account of the small number of trials and the sorts of eyes used. No indication is given of the degree of myopia in two of his four Ss nor of their ages. Bappert's Ss (contrary to Baird and Hillebrand) had difficulty in getting a sharp line for fixation. Bappert also confirmed Arrer's criticism that the visual field was experienced in different ways by different subjects, *i.e.* it might be seen as a single circular black-white disc or as two immediately contiguous semi-circular discs which moved back and forth, or the black half-disc might be viewed as a curtain ("Vorhang") which was suspended at different distances in front of a white background. Bappert in no place mentioned Baird's or Dixon's work although he must have been familiar at least with Giering's discussion of Baird.¹⁷ All of these criticisms apply just as much to Baird's and Dixon's apparatus as to Hillebrand's since their changes were largely in other respects. On the other hand, Bappert¹⁸ did not fully sustain Arrer's contention that fixation of either edge was impossible since their initial localization was indefinite.¹⁹

Mention should be made of a piece of work reported by Israel which was done at the Psychological Laboratory of Ohio State University under the supervision of A. P. Weiss.²⁰ The purpose of the investigation was to compare, at four low illuminations, the degree of accuracy, practice effect, and fatigue, in accommodation and convergence, in the discrimination of relative distances. A simple apparatus provided a mounting for two white beads about 6 mm. in diam., one of which remained at a standard distance of 75 cm., while the other could be moved about 10 cm. in either direction by means of a knob at S's hand. S's task was to set the adjustable bead at the same distance as the standard bead. The horizontal distance between the beads was 15 cm. Size and brightness were said to be ruled out by making one bead smaller

¹⁷ Giering (*op. cit.*, 78) called attention to one misunderstanding of Hillebrand's procedure in Baird's good summary of Hillebrand's work. Baird (*op. cit.*, 192) criticised Hillebrand's technique in moving the screen before the shutter was opened for the observing S. What Hillebrand says (*Zsch. f. Psychol.*, 7, 1894, 118) is that often the screen had moved 20 cm. before S noticed any movement, which is precisely what occurs in Baird's results, although not to the same extent. Such an error is easily understood.

¹⁸ Bappert, *op. cit.*, 174.

¹⁹ Cf. footnote 27 below.

²⁰ H. Israel, Accommodation and convergence under low illumination, *J. Exper. Psychol.*, 6, 1923, 223-233.

than the other, and by removing the gloss from one bead. It is said that S's attention was directed to these facts in order that his judgment might not be based on size or brightness differences. There was no control of head movement. Israel assumed that shielding one eye automatically eliminated convergence. No account was taken of binocular disparity and the results for the two eyes were considered as a function of the effectiveness of convergence and accommodation while that of one eye alone was considered as a function of the effectiveness of accommodation.

Swenson's work also had as its purpose a comparison of the factors of accommodation and convergence. The apparatus used was a modified form of angle-tubed stereoscope designed and constructed by H. Carr. The fixation-object was the combined image of two exactly similar lighted opal-glass screens. These screens were movable, and were intended to provide for alterations in accommodation while the visual angle and the intensity were held constant. Movement of the tubes also provided for alteration of convergence independently of accommodation. S indicated the position of the combined image by moving a concealed pointer on a rod which projected straight back from the apparatus along the line of vision.

Swenson secured results for three distances; 25, 30, and 40 cm.; first, for normal combinations, and secondly, for the six possible dissociated combinations of accommodation (A) and convergence (C) for these distances. Highly accurate estimates were made under the normal conditions of A and C combined. The estimates in the group-averages were always less than 1 cm. from the true distance, the deviation within the group was very small, and the individual sigma values, moreover, are given as 1.16 to 2.05 cm., 120 to 240 judgments. The group means for the associated combinations were equally consistent within the group. The values for all averages, individual as well as group, apparently show two things from which Swenson drew his conclusions: (1) without exception, under the dissociated combinations the means were located between the position for A and that for C; and (2) without exception, the means were nearer in value to the setting for C. Swenson concluded from (1) that both A and C. were effective and from (2), after mathematical treatment of the data, that C was, on the average, three times as effective as A.

In his treatment of the subject Carr has reviewed Swenson's work and has given two criticisms which must have been raised by Swenson's procedure; the inadequacy of the opal-glass screen as a fixation-object and the pin-hole size of the eye-aperture. The latter objection was met in the repetition of the experiment with a 5/16-in. in place of the 1/16-in. aperture. The fact that the results were not changed to any significant degree might indicate that with the pin-hole aperture, as clear images as were possible with such a diffuse light were obtained without the use of accommodation and that with the 5/16-in. aperture only diffuse images could be obtained. Accommodation, therefore, was inoperative in both cases.

A more serious objection to the procedure concerns the 7/16-in. disks within the tubes. These disks served to keep constant the size of the visual angle of the light-object. They remained, therefore, always at a constant position as the light screens were moved back and forth with the intention of changing the accommodation. While the Ss were told to fixate the center of the light objects, this rim, less than 1/2-in. in size, at this constant distance must have influenced the fixation. Indeed, in most of the experiments in which light-objects were used (*e.g.* Bappert) care was taken that the rim should afford such a confining region of fixation.

It is, however, the unpublished data of Mr. Duncan, whose work Carr par-

tially reports,²¹ which throws most doubt upon the results which Swenson secured.

Duncan's investigation, using the same apparatus and the same standard distances for A and C, was concerned with the effect which differences in size and intensity of retinal image might have upon the estimates of distance which the Ss made with the concealed pointer.

Under Swenson's standard conditions, the Ss' mean estimates varied less than 1 cm. in every case from the true distance and, as has been said, the sigma values varied from 1.16 to 2.05 cm. In Duncan's procedure, with variation in size of the light object, the Ss' mean estimates varied as high as 13.64 cm. from the true distance. With 3 different intensities of light-object, this variation ran as high as 15.74 cm. from the true distance. Duncan's disk of 10-mm. diam. comes nearest in size to Swenson's (about 11 mm.). With this 10-mm. light, the mean estimates of Duncan's Ss varied as much as 9.7 cm. from the true distance, whereas with the 11-mm. light the mean estimates of Swenson's Ss, as has been said before, were always less than 1 cm. from the true distance. No sigma values are reported for Duncan's work.

Such variability as Duncan found in his Ss' results would have made it impossible for Swenson to use those results as standard of reference, as he did in his own procedure when A and C were dissociated. We cannot assume moreover, that the size of disk used and the intensity used in Swenson's experiment were absolute or were inspired. Carr says "this close approximation (Swenson's) is due in part to the fact that an appropriate size and intensity of the light was employed."²²

Critical summary. The main general criticisms of the previous investigations may be summarized under the following headings:

(1) *Inadequacy of the fixation-object.* The screen edge, used in several investigations, was a doubtful fixation-object, on grounds already indicated, particularly when the fields of which it was the dividing line alternated black and white as they did in all but Dixon's modification of the apparatus. So far as illuminated opal-glass surfaces are concerned it is very doubtful whether they afford any challenge to fixation even when the rims confining these surfaces are taken into account. An adequate fixation-object must be small enough, at least in one dimension, and sharply enough defined, to produce a readily appreciable difference in size, under conditions when it appears in diffusion circles and when it is perfectly clear. Images of objects, moreover successively offered at different distances, for the purposes of depth-discrimination, must exactly replace each other on the retina, so far as the objective situation is concerned. The size, intensity and position in the field of view must be rigidly controlled to this end. Directing subjects to disregard size, brightness, etc., cannot possibly take the place of such control.

(2) *Means for illuminating the fixation-object.* In all the procedures with the screen edge, the illumination provided was indirectly from a large opal-glass background which reflected the light from lamps at the sides and in front of it. It is difficult to see how the two black screens at different distances from the background

²¹ Carr, *op. cit.*, 261-266.

²² Carr, *op. cit.*, 250. Swenson's lights show no comparable calibration with Duncan's. "An intensity of light was so chosen, and kept as constant as possible throughout the experiments, that it would be clearly visible yet not too fatiguing to the eyes" (Swenson, 371).

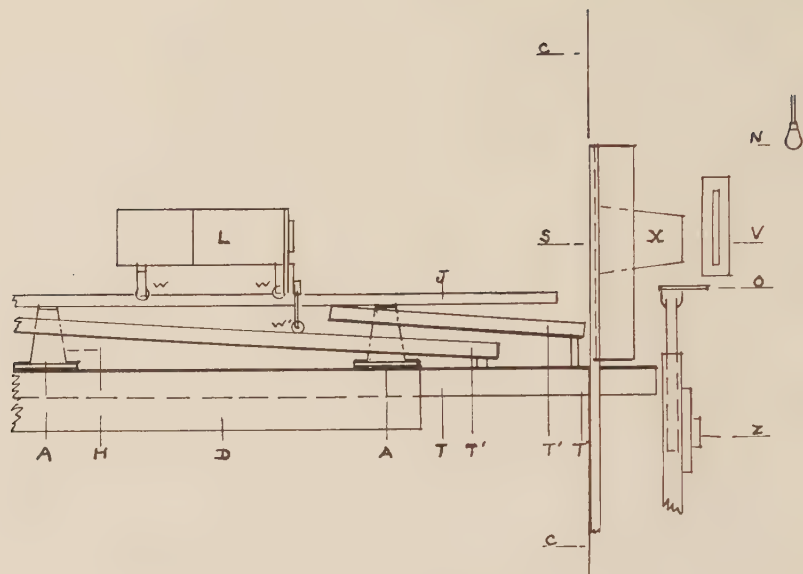


FIG. 1. LATERAL VIEW OF APPARATUS

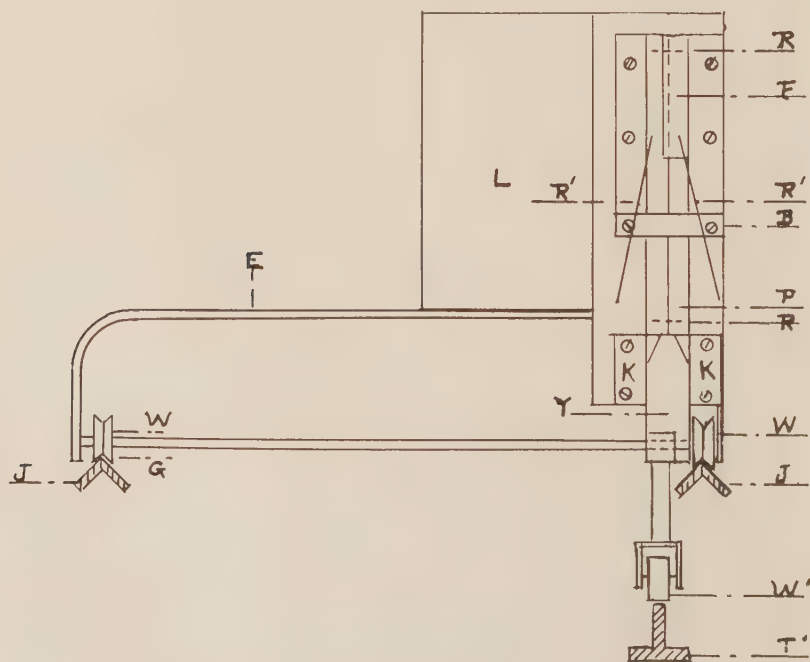


FIG. 2. DETAIL OF CARRIAGE AND FACE OF LIGHT BOX

could be made to appear equally dark. This criticism applies to the rods used by Giering and by Bappert as well as to the screen edge. Any sort of illuminated background, moreover, unless it be an absolutely textureless surface, introduces a point of reference from which the fixation-object may be localized.

(3) *Subjects.* Few *Ss* were used in most cases, and either their age and eye refraction were not given or, in cases where indicated, they were not suitable for testing accommodation, where that was the purpose of the investigation. Wearing correcting glasses, moreover, changed the situation markedly. In many cases the *Ss* were graduate students or adults of professorial or doctoral rank, particularly in the German studies. This fact insures considerable knowledge of the factors involved. In addition to the fact also that many eyes were ametropic, some also were presbyopic or approaching presbyopia.

(4) *Procedure.* (a) In any case where the head-position was not rigidly controlled, the results cannot be trusted, as Bourdon has shown. (b) In some cases results are presented on the basis of as low a number as 10 or 12 trials. (c) In almost all cases the exposure-time was not controlled. *S* in many cases was told to close his eyes between exposures, to look away, to give a signal, etc. (d) In some investigations *S* was informed concerning his errors at the end of a series of trials.

(5) *Approach.* (a) We observe a disposition to regard the main problem of the investigation of monocular depth as centered about the physiological factors of accommodation and convergence, and therefore a problem which must be settled absolutely in the negative or in the positive in regard to those factors.

(b) Experimenters have disregarded diffusion-circles on the ground that they gave no information concerning relative depth. Hillebrand, it is true, made use of diffusion circles in his conclusions but only as secondary to his "conscious impulse of will." Ascher spoke of some kind of empirical cue which might be effective and which depended upon the absolute distance of the object, but he evidently did not consider diffusion-circles.

(c) The fact that ciliary muscle is smooth muscle has been overlooked.

Apparatus. The apparatus used was adapted from Dr. Lloyd S. Woodburne's arrangement used in study of binocular parallax.²³ Extensive changes were found to be necessary for a study of monocular depth. The apparatus will be described as it was finally assembled.

The apparatus was constructed upon a steel frame consisting of four steel cross arms mounted upon a 2-in. T-iron rigidly supported at each end and placed at a height of 104 cm. from the floor. Four pairs of steel hangers (H), two of which are shown in Fig. 1, were mounted at proper intervals on the frame to carry a pair of brass tracks (J) made of $\frac{3}{4}$ -in. brass angle-strip. The possibilities of adjustment provided that the tracks should be held always exactly in line, exactly level and exactly parallel to each other at a distance of 20 cm. apart throughout the entire length of 175 cm. The light box (L) was moved upon these tracks by three brass wheels, two of which were in line upon one track, and the third wheel upon the outer track, thus forming the points of a right angled triangle. With this carriage and gauge of track, it was possible to prevent any variation in the light from a constantly vertical position at any point throughout the entire length of the tracks.

²³ L. S. Woodburne, The effect of a constant visual angle upon binocular discrimination of depth differences, this JOURNAL, 46, 1934, 273-286.

The wheels were $\frac{3}{4}$ -in. in diam. and were grooved to fit the triangular tracks (Fig. 2, G).

The slit-mechanism which furnished the light source remained practically the same as described and used by Dr. Woodburne and is shown in Fig. 2.²⁴ The extremes of opening (in our experiment) were from 1.8 cm. x 0.3 mm. to 4.5 cm. x 0.75 mm., an increase of 2.5 times; *i.e.* if the smallest opening was used at a distance of 80 cm. from *S*, the largest possible opening would give the same retinal image at 200 cm. The light intensity as well as the size remained constant since, with a constant retinal image, the amount of light entering the eye remained the same at any distance, exclusive of the possible effect of pupillary change.²⁵

The mechanism by which the slit-variation was accomplished is shown in Fig. 2. The inverted brass T-bar (*T'*) was so inclined that at 200 cm. it was 2.7 cm. higher than it was at 80 cm. from *S*. This represents the difference between the heights of the light-slit openings at these two positions. A wheel (*W'*), mounted upon a leg extending down from the brass plate (*Y*) and running upon the inclined track, served to push the brass plate upward through a pair of guides (*K*) and against the bottom of two vertically sliding steel plates (*P*). The sides of these plates and of the frame in which they moved were milled at an angle to allow for proportional changes in slit-width as the height increased, and small springs were mounted inside at top (*R*) and bottom (*R*) of the plates and pushed the plates apart to the width corresponding to a particular height. Longer coil springs (*R'*) insured the proportional decrease in size as distance from *S* decreased. The illuminated portion of the slit-mechanism was the extent of a line which was always the distance between the cross bar (*B*) and the overhanging flange (*F*). The light-source was a neon tube coiled within the light box and properly connected through a transformer to the 110-v. light-circuit. A Meumann time-sense apparatus with contacts properly placed gave a warning signal every 36 sec. The warning signal was followed by two exposures of two sec. each with a 2.4 sec. interval between exposures, during which the box was moved by the operator to its second position.

A wooden screen (*S*) 18 in. square, painted black on the side toward the instrument and tinted light red on the side toward *S* was placed at the end of the instrument-frame, but upon a separate wooden standard. An extended wooden eye-piece (*X*) was fastened to this screen and was painted red outside and lined with black velvet. The opening toward *S* measured 11 x 6 cm. This, however, was filled in with a tightly fitting piece of wood with a hole in the center the size of an optician's lens. Metal strips were arranged to hold such a lens when it was desired to use one. Over this lens-holder a removable black velvet cover was fitted with a small elliptical opening in the center. It was therefore possible to use a lens without *S* knowing that one had been placed behind the covering. It was also possible to insert a metal disc with a pin-hole opening or one with a 3-mm. opening to serve as an artificial pupil.

Two electric lights were placed inside two tin reflecting strips which shielded the direct rays of light from *S* but illuminated the whole face of the screen with a reddish glow and therefore kept *S* not only moderately light-adapted but also adapted to the red of the stimulus-light. On both sides of the eye extension-piece, metal-wire frames were attached in which mirrors could so be placed that an observer standing beside

²⁴ See Woodburne (*op. cit.*, 276) for detail of exposure-slits in his pair of boxes mounted on narrow gauge tracks.

²⁵ R. Jaensch, Zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol., Ergbd.*, 4, 1909, 8.

S could watch and record movements of the eye which was not being used for observation of the light-source. In order to get a more accurate and a permanent record of the eye movement a motion-picture camera was used with 2 Ss.

The mouth bit (O) as used at first was mounted on a frame attached to the instrument-frame. The slight vibration caused by the moving light-box was noticeable through the teeth. It was therefore removed and mounted upon an iron framework solidly attached to the floor but having no connection with the instrument-frame or with the screen. This frame was so built that it allowed a complete and accurate adjustment both vertically and horizontally for the mouth bit. The head-position then was constant for all observations at any one sitting.

In the eye extension-piece on the instrument side and therefore about 25 cm. from S a holder was arranged into which a sighter could be slipped, to be used in placing S for any given set of observations. This also served to hold the Wratten light filter (no. 29) which was used during the observations. The sighter was a piece of galvanized iron painted dull black and with a hole 3 mm. in diam. at the exact point where light-rays from the light-source would pass into S's eye no matter what the distance of the source.

The instrument extended into the corner of the dark-room so S sat in a triangular recess behind a heavy black curtain extending from the ceiling to the floor. Two buzzers (Z) were mounted low in front of S upon a board which was fastened to the mouth-bit frame. The tracks were covered with heavy black sateen both to reduce noise and to prevent reflection. The light-box and all other parts of the instrument, except the slit-mechanism itself, were painted dull black.

Preliminary experiments. Preliminary trials involved the use of 7 Ss, all undergraduates under 25 yr. of age. Four Ss were slightly ametropic, and one with -2.00 and -1.00 D. myopia observed with correcting glasses. One S had one normal eye, the other having been removed 10 years before. One S was functionally monocular because of a serious amblyopia. The seeing eye was slightly hyperopic.

Results with the two monocular Ss, presumably well practiced in monocular estimation, differed markedly from each other, even on an apparatus from which all the empirical cues were not yet excluded. The S with one eye was very keen in the use of slight cues which escaped the notice of any other S. The slightest noise, vibration or deviation in the position of the light either laterally or vertically was immediately detected. The amblyopic S could not tell relative distance on these or on any other cues. Similar differences were seen among the 5 Ss with approximately normal vision. One S reached a high percentage of accuracy largely on a basis of slight color-differences, *i.e.* the nearer light was redder, the farther light was more orange. This was attributed to a slight chromatic aberration and was responsible for the introduction of the use of the Wratten filter. The four remaining Ss showed little accuracy even with the presence of empirical cues. After considerable 'practice' their results still seemed to depend upon chance. In one of the last trials, one of these 4 Ss seemed to have a sudden insight into the significance of the initial blur of one of the lights and the number of errors in that and in the succeeding trials dropped appreciably.

Main experiments. The foregoing preliminary survey was fruitful in eliminating inaccuracies in the early adaptation of the Woodburne apparatus, in improving procedure, and in securing a closer definition of the problem. We now go on to the main body of the experiments.

At the beginning of every series *S* was so placed that every observation would be made from exactly the same position. The mouth-bit was adjusted level and in such a place that the observing eye was directly in line with the object-light wherever it was placed in the field, as determined by the passage of the light-rays through the 3-mm. sighter opening in the holder at 25-cm. distance from *S*'s eye. The light was on continuously in the otherwise totally dark field during this adjustment. Care was taken that *S* never saw the object-light in any way except in its setting in a totally black field. After an adjustment was secured which was known to be satisfactory for all positions to be given, the sighter was removed and the filter was inserted in the holder. *S* was instructed to place his teeth securely into the mouth-bit as soon as the warning-light appeared, to turn on the buzzer, and to wait for the appearance of two exposures of the object-light. Ample time was afforded for this in the interval between warning and first exposure. There was a 25-sec. interval for recording and for rest between successive trials.

S's task was to judge whether the second light was nearer than (N), farther than (F), or the same distance as (=), the first light. This record was entered in one column on a data-sheet. In a second column *S* added his certainty as C—, C, C+. Additional space was provided on the data-sheet for reports as to basis of judgment. At the end of the series he added other comments as he desired.

A series consisted of 20 judgments with one eye; a pair of series (making up a setting) consisted of 20 judgments with each eye separately in succession. There were two groups of series, a general group, and a short supplementary group for each *S*. In every series the distribution was 8 Ns, 8 Fs and 4 =s in a mixed order. Results are plotted in numbers of errors in a series of 20 trials. The upper set of curves for each *S* is in terms of the total number of errors in the Ns, Fs, and =s given in each series. The middle set of curves for the *S* is in terms of Fs alone (F), the lower set, of Ns alone (N). The curve for =s is superimposed in a dotted line upon the curve for the total errors. Since no essential difference was found in analysis of results from the two eyes, the trials are thrown together in the succession in which they occurred.

The following pairs of distances were used;

80 cm.—200 cm.,	representing .75 D. change
25 cm.—33½ cm.,	representing 1.00 D. change
33½ cm.—50 cm.,	representing 1.00 D. change
25 cm.—62½ cm.,	representing 2.40 D. change
40 cm.—100 cm.,	representing 1.50 D. change

The original plan was to start 2 *S*s on a far-pair of distances and work toward a near-pair; 2 other *S*s on a near-pair of distances to work toward a far-pair. It was thought by this means that practice might be equalized in both directions. Two other *S*s were never to be allowed to become familiar with any one pair of distances. As the work progressed, it seemed advisable to modify the procedure. Three *S*s remained on one pair of distances for a relatively long period of time with only short periods on other distances. With the remainder of the *S*s the plan was carried out as indicated above.

The pair of distances 40 cm.—100 cm. was given as the supplementary group of series to all *S*s save I.S.²⁶ Several weeks intervened in every case between the

²⁶ An artificial pupil 3 mm. in diam. was used here with all *S*s.

regular group of series and this supplementary group. In this latter procedure, the attention of *S* was deliberately directed upon any possible existing activity within the eye, with particular reference to any strain, effort, fatigue, or tension of any sort. In the regular procedure, no such reference was ever made. The *Ss* were never informed of any change of distance or in procedure. They were never informed concerning the accuracy of their judgments. All *Ss* made the observations in substitution for term-paper requirements in an elementary course in Psychology. No *S* wore correcting lenses except where noted in the text.

S	Class	Sex	Age	Refraction	No. of Reports	
					Gen'l ser.	Suppl'y ser.
JP	Grad.	F	24	+ .25	2000	200
EM	Soph.	F	18	+ .25	820	120
CW	Soph.	F	19	+ .12	700	200
WS	Sen.	M	20	R-25-25×105 L-25-25×75	1040	120
IS	Gard.	M	26	+ .12 + .25×90	700	
SW	Soph.	M	19	+ .12	1080	160
MH	Soph.	F	19	- .25	780	280

Results. The issue of the experiments is set down in detail for the 7 *Ss* considered one by one.

J.P. (a Chinese graduate student) made judgments distributed as follows:

A 280 judgments on several distances (before refraction).

B 920 judgments 33½ cm. and 50 cm.

C 520 judgments 33½ cm. and 50 cm.

D 280 judgments 25 cm. and 62½ cm.

E 200 judgments 40 cm. and 100 cm.

Curves for B, C, D, and E in Fig. 3.

The first group (A) seemed at the time to be unsatisfactory on account of persisting blurs, apparently at any pair of distances. In the first pair of series when urged to get a clear image, *S* wrote that she tried to do this but that it interfered with her judgment. Judgment was based on blur, was uncertain, and errors ranged from 5 to 18 in each series of 20 trials. The trials were discontinued until after a refraction was made. When refraction showed only 0.25 D. hyperopia, observations were resumed.

In the first series of trials in the second group (B), 15 out of 20 lights were blurred for her on the light which was offered at 50 cm., *i.e.* the far-position. *S* made her judgments on the basis that the nearer light would be the blurred one and this accounted for 15 of the 17 errors. If there was no blur she judged the lights to be equal. In the second series at the same sitting only three lights were blurred throughout the exposure, one far and two near. Blurs decreased rapidly until in the sixth series there were no persisting blurs. Through all the succeeding series in group (B) she could give no basis for her judgment other than that one light seemed occasionally to come in front of the other. In a series with 9 errors she expressed no more certainty than in the preceding series, which contained 16 errors. In the two following series, however, with 6 and 4 errors she was more certain but still had no basis for judgment. In the 16th trial she indicated (correctly) that, of

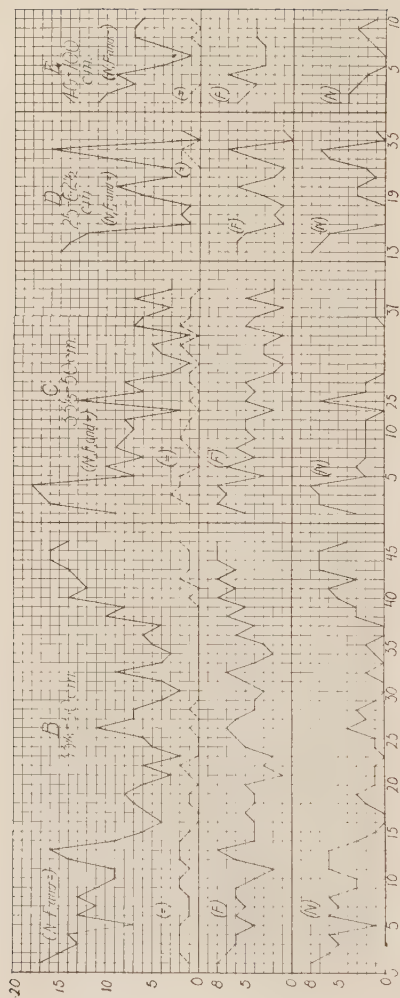


FIG. 3. RESULTS OF J.P.

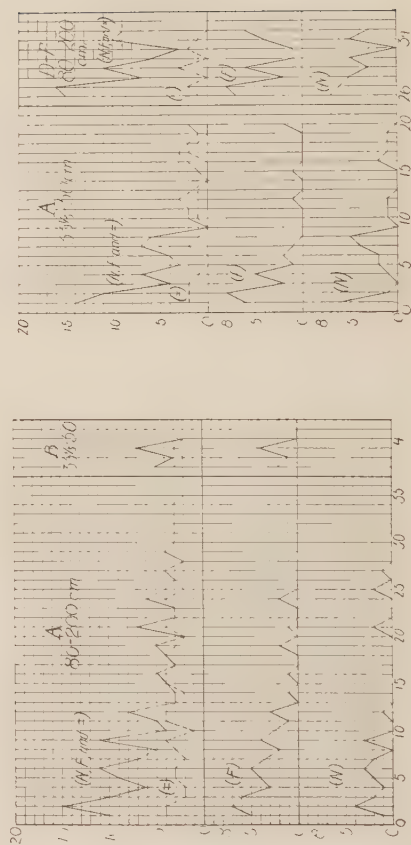


FIG. 4. RESULTS OF E.M.

FIG. 5. RESULTS OF C.W.

3 successive equals given her, the first 2 were of a farther light, and the third of a nearer. In the 21st. series, on suggestion, she placed in brackets on her data-sheet those experiences which seemed similar to her. These were mainly correct, but in one case included 4 successive equal judgments for as varied procedures as the following:

- (a) *Near* light at $33\frac{1}{3}$ cm.; then *Far* light at 50 cm.
- (b) *Far* light at 50 cm.; then *Near* light at $33\frac{1}{3}$ cm.
- (c) *Both* lights at $33\frac{1}{3}$ cm.
- (d) *Both* lights at 50 cm.

One month intervened between the end of the series represented under B-Curve and the beginning of those represented under (C) on the same pair of distances. The first pair of this series showed 9 and 16 errors. Before the next pair of series the problem was discussed with S, *i.e.* the facts of accommodation, of possible strain, of fixation, of focus, etc. She was asked to fixate and get a clear monocular image, of a pencil at different distances, and to think of the experience in terms of the eye itself rather than of the image or the object. In the next pair of series the errors were 17 and 18 out of the total of 20 trials each. Her comments were to the following effect: The procedure hindered judgment. She could not tell what was strain and what was not strain. When asked to localize this 'strain' she was puzzled, but finally decided that it consisted in squinting up of the eye and closing the lid. She could not decide whether relative closure or relative opening of the eye formed the strain. In this pair of series, she reported 'strain' in, respectively, 11 and 10 trials, but with only two correct judgments in the whole number. She seemed, however, to establish here a formula which she continued to use later, namely, 'strain' on the first light indicated that the second light was farther and *vice versa*. This formula, if it was used, was not effective in reducing errors in this pair of series. However, upon the same basis in succeeding pairs the number of errors dropped gradually until, in Series 12 the errors were of 2 fars with all nears and equals correct. These fars were in each case called equal, a confusion which accounts for practically all of her errors when a far-light was given second.

In order to see if she would transfer immediately this use of 'strain' to another pair of lights, she was transferred on the afternoon of the same day to observation of distances 25 cm.—62.5 cm. (Series 13 under D-Curve). The errors were 15 and 14 out of 20 trials each, and all upon Ns and Fs. The lights offered successively in the same position were judged correctly. All the errors were on the basis of strain; *i.e.* on her basis the light at 62.5 cm. was the one which gave 'strain' and was therefore (incorrectly) judged as nearer. Her comments were: "These series caused much strain on the eye. All the images became clear but it took an extraordinary amount of effort to get a clear image. The strain seems to be more than in the eye lid, but I cannot say where."

The next pair of series took place the following day, that with one eye giving 12 errors, with the other 1 error. Within the first of these series there is plainly recognizable a clarification of the idea of the effect of 'strain.' *i.e.* she seemed to become conscious of strain in the direction in which it might be entirely consistent (granting strain) with the pair of lights offered to her; nevertheless, she either interpreted this wrongly or still lacked the proper N, F or = interpretation for it. On the second series of the pair, on the contrary, the interpretation is correct in all instances except on a far-light which she called equal (or far). Since with a double judgment only the first was counted, this was called incorrect. Curiously enough in

every case and in the general comment at the end she reported "very slight strain" and this was always indicated as accompanying the light offered at 25 cm. She also selected all fars and placed them in brackets as giving her exactly the same experience (including the one in error above). The eye lid was still described as the source of strain, but she added that when she squinted she sometimes felt that the "inside of the eye was different" but was not sure. She thought she knew when there was little or much strain but could not localize it any more closely.

After continuing with the same degree of accuracy for 3 series (16, 17, and 18, D-Curve) a disk with a pin-hole of slightly less than 1 mm. in diam. was placed in the lens-holder. The two resulting high points, that of Series 19 and 20, and of 23 and 24, D-curve resulted with the drop between, when the pin-hole was discarded. She became again very uncertain and with no basis for judgment. This was the effect which was expected, since diffusion circles and necessity for accommodation were both eliminated.

With the next pair of series *J.P.* was transferred back to the distances $33\frac{1}{3}$ cm. and 50 cm. used previously. The high point in the middle of the curve, Series 25 under C, represents the first series of this pair. In successive trials, as may be seen in the curves, after a brief adjustment to this distance the *Ns* were practically always given correctly. The errors on *Fs* usually resulted from a confusion with ∞ s. Confusion of far with near occurred in only 4 cases out of 72 far-judgments. These were given on the basis of strain on the second light, a mistake which is easily understandable. A pair of series was introduced here (Series 35, 36 in D) again on the distances 25 cm. to 62.5 cm. to see if the shift would cause much disturbance. These showed 2 errors out of 40 trials, a near and a far both judged equal. With an assistant to operate the instrument, *E* watched *Ss* unobserving eye through these 40 trials. In a few cases convergent and divergent movements were noted unmistakably and a later check showed that these corresponded to the objective position of the lights. In most cases, however, these were difficult to catch for at least two reasons: (1) There was a great deal of lateral sporadic wavering of the eye, at least before and between the light exposures; (2) The eye lid actually 'screwed up' during the light-exposure, especially on the nearer light.

The last 4 series were again upon the distances $33\frac{1}{3}$ cm. and 50 cm. The first of these, Series 37, contained 3 errors. The formula S_1 (strain on the first light) and S_2 (strain on the second light) had been used by the *S* so persistently upon her data-sheet that *E* asked her which she usually indicated first, the strain or her judgment. She was not sure which came first in the larger number of cases, but agreed to watch this. This task seemed to cause distraction and the errors amounted to 7 in this series. It was shown that, at least in this series, the judgment was usually given first. The next and final pair of series dropped again to 3 errors each. Strain was still as indefinitely localized as before.

The supplementary series of 200 judgments at 40 cm.—100 cm. showed that an adjustment period similar to that of the previous distances seemed to be necessary here. In one series also, *S* made 14 errors because she applied her formula S_1 , S_2 in the wrong direction. She corrected this before the end of the series. (The supplementary series does not have the same significance with this *S* as with the five succeeding. As noted before, the discussion of strain, of accommodation, and the demonstration of successive monocular fixations was introduced with *J.P.* at the third trial of C, instead of with the supplementary series.)

E.M. made judgments distributed as follows:

A 700 judgments 80 cm. to 200 cm.

B 80 judgments $33\frac{1}{3}$ cm. to 50 cm.

C 40 judgments 25 cm. to 62.5 cm.

D 120 judgments 40 cm. to 100 cm.

Curves for A and B are shown in Fig. 4.

Examination of A curve shows a steady decrease in errors except on the lights which were shown twice in the same position. The superposition of the = Curve upon the total shows this clearly. From the first set of trials her judgment was increasingly based upon what she called 'focus.' Of the errors occurring in these series, 40% occur on this basis; 12% of these are *Ns*, 41% *Fs* and 46% =s.

Lights which were shown twice in the same position show a high percentage of errors. In 31% of the series every one of such positions (namely, 4) were in error: in 42% of the series 3 out of 4 were in error. In no series were all equals correct. The basis for these judgments also were in terms of 'focus.' Analysis of the errors on these lights presented successively at the same distance, shows

$35\frac{1}{2}\%$ given at 80 cm. were judged *N*.

$26\frac{1}{2}\%$ given at 200 cm. were judged *N*.

7% given at 80 cm. were judged *F*.

$6\frac{1}{2}\%$ given at 200 cm. were judged *F*.

$7\frac{1}{2}\%$ given at 80 cm. were judged = (correct).

17% given at 200 cm. were judged = (correct).

Where this *S* had an 'equal' judged correctly, it was always on the basis of "no change in focus." Where 'equals' were judged incorrectly as *F* or *N*, the basis was given "came in back (or front) of focus."

A small group of 80 judgments in 4 series was taken at the distances $33\frac{1}{3}$ cm. and 50 cm. These are shown in B-Curve. There were 17 errors in this set, 7 of which were upon the equals. No errors upon the nears. A succeeding 40 judgments on the distances 25 cm. and 62.5 cm. gave only one error and that upon an equal.

E assumed at first that these remarks on focus referred to the eye. Later results indicated that *S* usually spoke in terms of the image projected rather than in terms of any activity in the eye. No strain or effort was ever indicated. In 2 series (2nd. and 13th.) the following remarks occur: "feel eye focusing closer," "felt refocusing up nearer." These two remarks on two single occasions, accompanying one correct and one incorrect 'near' judgment are the only instances when the judgment was given clearly on the basis of any possible activity within the eye. Focus to her had apparently no relation with an activity within the eye, but referred to the appearance of the object perceived. One of her later remarks indicates this: "Near ones still have that out of focus appearance at first glance." She described focus somewhat in the same manner as the spectator who looks at a stereopticon projection and refers to it as in or out of focus.

In the supplementary group of 120 judgments, the only errors were upon the equals (16 errors in 24). These errors show the same preponderance of equals judged near, as in the description of the general group. All correct equals, as well as those in error, were given with C— certainty, whereas all nears and fars, except in two single cases, were given with C+ certainty. In her remarks *S* remained quite as puzzled as before in regard to any more definite reportable basis. Attention directed

upon the eye seemed to be of no assistance in further determination of this basis. Some of her comments are the following:

Movements of the eye are distinguishable. A slight turning toward the nose. Lights are called further or nearer because they seem to correspond to former experience with both eyes.

In this trial the farther ones were more easily judged than before by the movement back. (She could not tell what this meant except that it is "the same feeling as in judgment with two eyes.") With the nearer ones the appearance of the image, that is, the fuzzy look when it first appears, interferes with the recognition of eye movements.

The strain of change of focus seems to be of a different sort for nearer than for farther. It seems to be partly a turning-in feeling and seems to be stronger than the feeling for moving the focus farther away.

Judgment is based on the image rather than upon eye movements; whether the line looks farther or nearer rather than any feeling in the eye.

C.W.'s judgments were distributed as follows:

A 400 judgments 33 $\frac{1}{3}$ cm. to 50 cm.

B 80 judgments 25 cm. to 62 $\frac{1}{2}$ cm.

C 20 judgments 33 $\frac{1}{3}$ cm. to 50 cm. (with a +2-D. lens)

D 120 judgments 80 cm. to 200 cm.

E 40 judgments 25 cm. to 62 $\frac{1}{2}$ cm.

F 40 judgments 80 cm. to 200 cm.

G 160 judgments 40 cm. to 100 cm.

Curves for A, and D-F, are shown in Fig. 5.

C.W. illustrates clearly the rise of a deliberate basis for judgment. At the end of the second set of trials she wrote:

There is obviously a different amount of focus for a near and for a far light, because the amount of blur is different, but I do not know which is which—only to record equal if the focus does not change much. If I had time to figure out what the blur—and focusing—mean, my results would be consistent and certain, but not necessarily correct. As it is, I am apt to regard the light which appears with the least blur as the farther one. If the next one requires a good deal of focusing, it surely is not equal and I assume that it is near. I am assuming only two lights, I think.

As may be seen from examination of the first curve, there were 14 errors in this series with 11 errors in the succeeding series. By the next series (at the same sitting and with the other eye) she had adopted two methods of judging, both of which she used for several succeeding series and which she expressed as follows: (1) tried to hold focus between lights after deciding whether the first light was near or far; (2) tried to regain normal focus between lights and count that light farther which required least focus.

In the second pair of trials, 5 of the 7 errors with the left eye occurred on the basis of the first method. After a few more series S abandoned the first method in favor of the second because, "the lights are no longer as familiar by the amount of focus, but more especially, because there is too much room for error in this method." By the time she made this report her errors had become negligible and the initial blur was brief. This last is probably the reason why she reported that neither the lights nor the distances were the same in succeeding trials and considered, moreover, that at least three sets of distances were used. From this time on, the far-judgments were usually on the basis of "the second requires no focus" or "almost no focus"; and the near-judgments, on the basis of requiring "the same relative amount of focus from the normal."

In the first series, with the distances 25 cm. and 62.5 cm. (Series 21) *S* commented: "There seem to be only two distances now, one requires a lot of focus, the other practically none. The near one is very blurred at first. After an initial blur, however, the images are clear." In this group of 120 judgments she made only 3 errors and those were on *F*s. The curve for this set is not included. Some of her remarks on blur and focus caused *E* to ask just what she meant by blur and by focus and what was the relation between the two. She wrote that the focus was the amount of change necessary *in the image* to clear it up and that the blur was practically equivalent to amount of focus. "The amount of blur that disappears, in fact the change in the width of the band, I count as the amount of focus required to see it clearly." In Series 24 her comments were "the light which requires most focus and which I take to be the nearest is harder to look at, or is, at least, until it is focused. The eye which is not in position at the opening seems to feel the strain, or whatever it is, more than the other. When I mean to return to normal focus between lights I feel that the free eye moves a little outward, particularly after a near light." She reported this as true at least when the left eye was observing. This is her only reference to strain in the regular group of series.

The next eight series (under D-F in Fig. 5) were given upon the distances 80—200 cm. This pair of distances confused *S* greatly because "the amount of change of blur is difficult to see." Equals were often correct on the basis of no blur or the same amount on each. Here also she believed that three distances were used, one giving her a clear light, one a slightly blurred, and the third a very blurred light. The curve as far as the observations continued was a descending curve.

In the supplementary group of 160 judgments on the distances 40—100 cm., and with attention fixed upon the activity of the eye itself, *S* retained her high percentage of accuracy, with one error only on a near, 3 upon fars and 9 upon equals. She could write only in vague terms concerning the eye's activity. Again, it was the eye which was not occupied in the observations which gave the only indication of activity, so far as she could discover. Like *E.M.* she was sometimes vaguely conscious of a "turning in" movement. On one data-sheet she wrote emphatically "I'd swear it is the other eye I'm conscious of. It moves on near ones, moves in, with almost a cross-eyed feeling." Again she wrote that she kept trying to figure out what had happened, if it did happen, and said that if she did not have the other eye she might know what was happening in the observing eye. Also like *E.M.* she returned, apparently with relief, to her own method, previously described as change in relative amount of "focus."

W.S.'s judgments were distributed as follows:

- A 280 judgments 25 cm. and $33\frac{1}{3}$ cm.
- B 400 judgments 80 cm. and 200 cm.
- C 20 judgments 80 cm. and 200 cm. (with lens)
- D 160 judgments $33\frac{1}{3}$ cm. and 50 cm.
- E 80 judgments $33\frac{1}{3}$ cm. and 50 cm. (with lens)
- F 40 judgments 40 cm. and $62\frac{1}{2}$ cm.
- G 40 judgments 25 cm. and $62\frac{1}{2}$ cm.
- H 220 judgments 40 cm. and 100 cm.

Curves for A, B, D, F, G and H are shown in Fig. 6. The above distances were used in such an order that usually one pair of series only (one with each eye) was given in succession with any one set of distances. *S* was never allowed to become

familiar with any pair of distances. The first curve, (A, in Fig. 6) represents judgments given in numerical succession of pairs of Series 1-2, 5-6, 13-14, 17-18, 19-20, 25-26, 33-34. When the trend of results with distances 80—200 cm. was observed, that pair of distances had the emphasis slightly in the distribution of trials for the remainder of the observations. Refraction for this *S* could not be made until after the observations had been finished.

In the first pair of series at 25—33½ cm., size of image, position in the field, and clarity of image were given as the criteria of judgment. A general statement of basis was made after each series of the next pair at 40—62.5 cm. to the effect that when the second light was farther *S* got a clear image without effort, but when nearer some effort was required. In pair 5-6 (distances 25—33½ cm.) the above statement was modified to the following: "Basis is much less confused. When the second light is nearer, there seems to be some muscular contraction behind the eye pressing in. The second light is farther if this does not produce a clear image or if, at first sight, the image was more distinct." (Series 6 is omitted from Fig. 7A.)

Two pairs of series at the distances 80—200 cm. were introduced at this point (Series 7-8, 9-10, B-Curve in Fig. 6). Errors ranged from 10 to 17. *S* indicated the same basis of judgment as before, but added "On certain trials it seems as if a lens were placed before one or both of the lights, making it impossible to get a clear image." A pair of judgments at 33½—50 cm. and a pair at 25—33½ cm. intervened before the next pair at 80—200 cm. was given. These two pairs do not add any new fact except that on Series 13 (25—33½ cm.) *S* spoke of having very clear images of both lights. The remarks after the next pair (with 80—200 cm.) showed that *S* was puzzled. In the first set of this pair, *S* felt that there was some "tampering with the images," and he labeled four trials as surprising and "artificial" and gave no judgment on these. There were 19 errors out of 20 trials, including the four omissions. All judgments except these four, however, were given with high assurance and all images were clear. In the second set of this pair (at the same sitting, but with the other eye) judgment was given for all except two. Certainty in two cases, however, was C—, and one of these cases was a correct judgment. At this point (the 15th trial of the series) *S* spontaneously remarked that a revolution was going on, and that he wanted another series of trials immediately, in order to get hold of whatever might be the factor which was making him reverse his former judgments. Later he wrote "the general basis is the same but the eye-activity seems opposite to what had occurred before." The next trial was purposely five days later and on the distances 25 cm. to 33½ cm. again. His only remark was "same basis as before and the trouble of the last time is absent." With the second series of this pair he was asked to describe exactly what happened when the two lights were given him. This description is to the effect that a near light (*i.e.* what he believed to be a near light) comes in blurred but clears up; a far light comes in perfectly clear and in some way which he cannot describe, it just appears far. The greatest difficulty, for him, he believed, was to tell when they were in the same position successively.

The next pair, on 80—200 cm. (21, 22), showed 12 errors in each set with *S* puzzled as before. Two pairs which followed with other distances showed nothing new. The next pair of 80—200 cm. (27, 28) showed almost complete reversal of near and far. While preceding trials on these distances contained many instances of this reversal, it was by no means as complete as on this pair of series. He reiterated his statement that he was using the same basis of the way the eye reacts, but added that he "has a feeling every now and then that it may be reversed, that is, it looks

a bit the opposite—in some way looks different." The intervening pair ($33\frac{1}{3}$ —50 cm.) again showed nothing new. After the first series (31) of the next pair (80—200 cm.) he wrote: "More certainty with regard to judgment. The immediate clear-

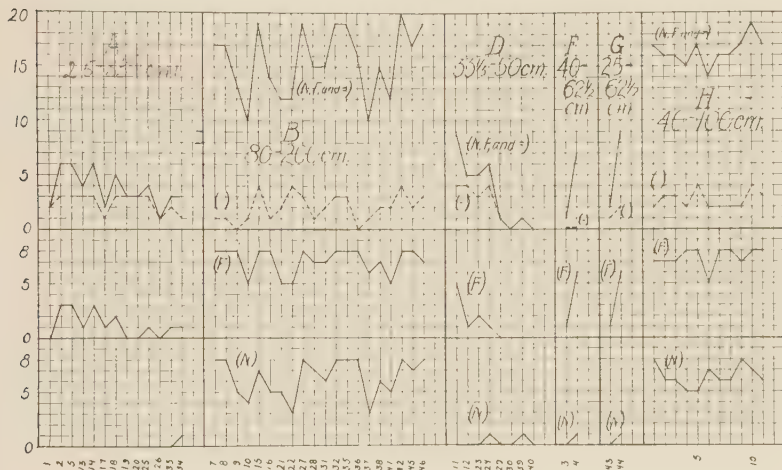


FIG. 6. RESULTS OF W.S.

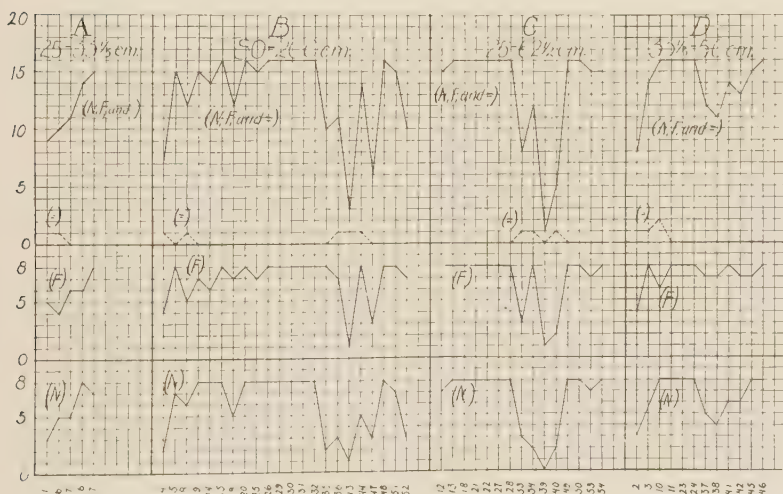


FIG. 7. RESULTS OF S.W.

ness of the farther image (when second) has perhaps thrown me off and caused me to see it as nearer—this applies to the other series of this type more than to this one." The farther image of which he spoke was really that of the nearer light. This complete reversal occurred in many of the succeeding series with these distances and accounts for the height of the curves in B. Occasionally the doubt returned and he

gave one or more correct judgments but with decreased certainty. The remainder of the trials shows nothing essentially new except five series which do not appear in the curves. In one pair of these a $+2$ D. lens was placed in the lens-holder, without his knowledge, when the distances $33\frac{1}{3}$ —50 cm. were to be presented. He gave complete reversal of nears and fars on both of these series with C+ certainty and his comment was "nothing new." Errors amounted to 17 and 19 out of 20. In the attempt likewise to reverse in the direction of correctness the 80—200 cm. distances, he knew through an accident in procedure, that a lens was before his eye. Out of the first nine single trials eight were in error, with, however, no errors on the remaining eleven. From his comments all of these seemed to be correct on the basis of comparative blur.

After the usual instruction, W.S. gave 220 judgments in the supplementary group, the curve for which is shown under H (Fig. 6). Eighty-two per centage of these judgments were in error and 88% of these errors were reversals of near and far in the same manner as that indicated in the B-curve with the distances 80—200 cm. Practically all of his judgments were given with C+ certainty. On the first data-sheet of this group, when it had been suggested that he observe the lights throughout the exposure-time, he indicated that it was not necessary to look longer at the lights since his judgment was immediate and was based on experience. In Series 3 and 4 single comments which accompany reversals, apparently indicate some influence of the factor of contradiction: (1) "Divided personality here," and (2) "Something terribly fishy about this." There were no other comments throughout the 220 judgments. He asked, however, for many trials to be repeated. Another trial was given in every case, but it was never an exact repetition of the next preceding.

In Series 25 and 26 an assistant examined this S's unobserving eye in the mirror adjusted for this purpose. Like J.P., the sporadic movements of the eye interfered to such an extent that no sure determination could be made of definite, unvarying convergence and divergence which corresponded to the objective positions of the light. Interference came from the pulsating pupil, movements of lids and of brows, in addition to what the assistant could only call "flashing of the eye" each time that the light appeared and challenged attention. All of this interfered with the ocular observation. The cinema-records made later of this S's eye show these same characteristics.

S.W.'s judgments were distributed as follows:

A 100 judgments 25 cm. and $33\frac{1}{3}$ cm.

B 440 judgments 80 cm. and 200 cm.

C 300 judgments 25 cm. and $62\frac{1}{2}$ cm.

D 240 judgments $33\frac{1}{3}$ cm. and 50 cm.

E 160 judgments 40 cm. and 100 cm.

Curves for A, B, C, D are shown in Fig. 7.

(Like W.S. this S was not allowed to become accustomed to any one pair of distances.)

S.W. apparently made his judgments in his early series on the basis of blur. "Closer lights appear slightly less definite, as would a pencil held close to the eye." As the trials progressed he gave an increasing number of equal judgments. The comments show a continually decreasing amount of blur accompanying the increase in errors.

Since *S*, even after 640 judgments, showed less indication of learning than *J.P.* did, it was decided to introduce the idea of strain, accommodation, etc., before Series 33 (25—62.5 cm.). Accommodation, possible strain, fixation, etc., were discussed and demonstrated. As a result, errors dropped markedly, five nears and five fars were correct on the basis that the near light caused "strain as if very near." On the afternoon of the same day, a pair of trials was given on distances 80—200 cm. A similar drop occurred here in the curve of nears but not in the fars. A lesser drop occurred in the next pair of distances ($33\frac{1}{3}$ —50 cm.).

The next presentation of distances 25—62.5 cm. (Series 39) was given 16 days after the instruction noted above. Only one error occurred and that was on a far light. 'Strain' was indicated after each trial; whether on both, on neither, or on one alone. The basis on which judgment was given was "definite strains noted above so strong as almost to cause physical pain." The second of this pair of series occasioned decreased strain and the errors were five. The succeeding pair (41, 42) ($33\frac{1}{3}$ —50 cm.) showed decreased strain and increased errors. The first of the next pair at 80—200 cm. (43, 44) contains three errors only, with all judgments on the basis of strain. *S* remarked, however, that strain was more apparent in other series. From indications of blur it became apparent that any light which caused a strain caused also a blur which persisted throughout the showing of the light. From this time on, however, the indications of strain decreased, the errors increased, and *S* returned to his judgment that all were 'equals.'

Two series of 20 trials each (not included in the tabulation above or in the curves) were given with a -1 D. or -1.5 D. lens to see if the increased accommodation necessary to overcome the lens would be indicated in strain and in decrease of errors. Outside of four correct equals, which are negligible on account of facts noted below, 4 nears were given correctly on the basis of strain, and the report is made that blur persisted throughout the showing of the light which caused the strain. *S* also remarked that he thought perhaps the blur was the more indicative of the two—he saw the blur and felt the strain.

The performance in the supplementary group (not shown in curves) in 120 judgments showed the same sort of thing. One series was made in which all judgments were given as equal. In the other 100 judgments 9 *Ns* and 9 *Fs* were given, and of these 3 *Ns* and 4 *Fs* were incorrect. These nears and fars were given on the basis of strain and each light was blurred throughout its exposure. All other lights were clear and distinct.

In Series 50 (25—62.5 cm.) *E* observed the unused eye in the mirror adjusted for this purpose, while an assistant operated the lights. Without knowledge of the combinations given by this assistant, in every case convergence and divergence of the eye were noticed which corresponded accurately to the distances given, as checked later with the assistant. Twice the lights were given in succession at 62.5 cm.; no movement of the eyes occurred. For two trials with lights given successively at 25 cm., the notation for one of these was 'a slight inward, outward, inward movement'; for the other, $I_1 I_2$ indicated a marked convergence on each light as it appeared. For 5 trials they are indicated as slight and for 9 trials as well-marked movements. These movements all accompanied judgments of equal as shown in Series 50, C-Curve, and were therefore disregarded entirely so far as the judgment of distance was concerned. No other *S's* eye was steady enough to give such unmistakable movements. Cinema-records made later of this eye were not as conclusive as this sort of observation.

The remaining two *Ss* (*I.S.* and *M.H.*) gave judgments as indicated in the table on p. 527. Curves are given in Figs. 8 and 9.

I.S. was not available for a supplementary group. The two low points of his curve at trial 11 and 34 are probably due to two errors in procedure. When these were

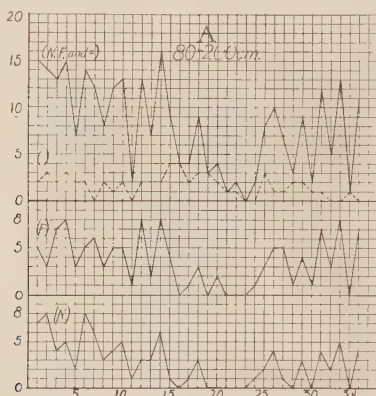


FIG. 8. RESULTS OF *I.S.*

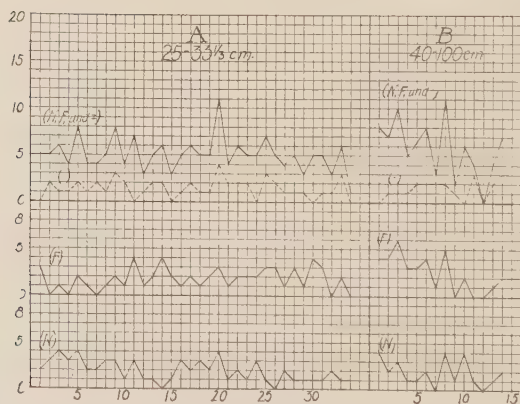


FIG. 9. RESULTS OF *M.H.*

corrected, his temporary accuracy was lost. *I.S.* mentioned blur only once. Difference in size of lights and in intensity were the only criteria he could find as basis for judgment. *M.H.* was able to give no basis for her judgment. She frequently reported 'thickness' of images which she said was not blur. Her errors seemed to be due to misinterpretation of this blur or 'thickness,' and correct judgments in most cases seemed to be due to an interpretation consistent with the position of the lights given. In the supplementary series with attention directed upon the eye, *S* found no assistance in clearer definition of her basis of judgment. She still reported some 'thick' images which she said were not blurred. She seemed to think that perspective played a part in her basis. She could find no indication of strain. There is some indication of a descending curve on these distances (see B-Curve, Fig. 9).

SUMMARY AND CONCLUSIONS

The reports and the graphic records show definitely that the experiment resolved itself into a 'learning' experiment. Deprived of the usual means of discriminating distance, the 7 Ss were faced with the necessity of finding new resources. Some of them found these, others did not. The curves show marked differences in performance.

Only the curves for *E.M.* and *C.W.* (Figs. 4 and 5) are closely similar. These are clearly 'learning' curves. The characteristics of 'learning' were just as definite for the pair of distances on which *C.W.* was engaged ($33\frac{1}{3}$ —50 cm. from the eye) as for the greater distances of *E.M.* (80—200 cm.). The basis of judgment for both rested upon what they called 'focus,' the word referring to the image and not to any activity within the eye. Analysis of *E.M.*'s judgments shows a rapidly increasing number based on 'focus' in the learning period, with a relatively rapid decrease in errors. During the descent of the curve, however, 40% of the judgments based thus were in error. *C.W.* showed the rise of a deliberative sort of basis for judgment in two methods, one of which she discarded. The basis retained was amount of change in focus, which she described as "equivalent to amount of change in blur." *C.W.* gave more correct reports than *E.M.* when the lights were shown one after the other in the same position. No other Ss so definitely and consistently compared the appearance of the light-object (amount of blur) at the various distances.

J.P. (Fig. 3) showed no effects of learning at the end of 960 trials, *i.e.* of more trials than *E.M.* or *C.W.* Instruction given *J.P.* in order to turn her attention upon any strain or activity within her eye showed its effects, not immediately, but in later portions of the curves. She slowly developed and applied a formula, that 'strain' on the first of a pair of lights meant that the second was farther away and *vice versa*. The application of this formula was temporarily disrupted by any change in pairs of distances. The high point at Series 25 (C-Curve) and those at the first series on each of Curves D and E, illustrate this interference. Such interference shows that 'strain' was not any immediate and unmistakable muscular sensation to which her attention had been called, but was related rather to the relative amounts of initial blur in the pairs of lights presented. Even with the pair (D-Curve) which involved as much as a 2.4 D change in accommodation, it was necessary for *J.P.* to re-establish her formula. It took 60 trials with 41 errors before she was able to do this. While she attained some degree of accuracy, she never reached, on any pair, the accuracy of *E.M.*, (*i.e.* no errors on Ns or Fs in 160 trials).

The significant characteristic of the curves of *W.S.* (Fig. 6) is that where distances of 62.5 cm. and beyond were involved S reversed N for

F in practically all cases. His large number of errors on Curves B and H is due to this reversal. With his slightly myopic eye the farther light in the pairs of distances 80—200 cm. and 40—100 cm. apparently came in more blurred than the nearer light. He therefore called it nearer. His judgments throughout, both before and after instruction, were based—as he affirmed—on “definite muscular movement” or “effort,” but his persistent reversals give sufficient evidence that the main basis was amount of blur. The occasional contradiction in his experience was overruled in the direction of error by his presupposition that greater amount of blur meant ‘nearer.’

The early parts of the curves for *S.W.* (Fig. 7) show increase of errors instead of decrease. The error-curve remained high for 640 trials, that is, until he was given special instructions. The drop in the curves which appeared almost immediately is indicated in C-Curve at 33 and 34, and on B-Curve at 35. After the instruction, lights were reported as blurred throughout these exposures and ‘strain’ was given as the basis of judgment. After a few trials, the blur disappeared, strain was no longer reported, errors increased, and the curves resumed their previous form. *S.W.*’s temporary accuracy, apparently secured by the slowing down of accommodation, could not persist on account of the usual very rapid adjustment of his lens. He therefore lost the only means he could have used for learning to discriminate, namely, comparative amounts of blur.

Neither *I.S.* nor *M.H.* (Figs. 8, 9) achieved any consistent accuracy. Neither one attached significance to the blur circles, although the latter frequently reported a ‘thickness’ of the light, and the former reported differences in size as a basis of judgment.

Conclusions. (1) A considerable period is necessary, with individual differences, before a single eye can accommodate accurately and achieve a clear image of a well-defined light-object which is isolated completely from any related context in space.²⁷ With untimed exposures in earlier experiments this fact has been obscured.

(2) The relative depth-localization of the object in this experiment did not arise immediately and directly with the clearing-up of the image, as it has been supposed. The situation took on the character of a ‘learning’ experiment where the most significant factor appeared to be the comparative amount of change in the image at different distances, a change which involved both clarity and size of the diffusion circles.

(3) Diffusion circles therefore play a much more considerable part in

²⁷ If a clear image were immediately produced, it would mean that some third dimensional relation was already discernible in the field, and that the object was not in complete isolation. This was probably true in some of the previous investigations.

estimation of relative distance, at least in experimental situations, than has previously been supposed.

(4) Another factor, probably proprioceptive, forms a part of the pattern involved in recognition of depth. This factor, however, may be overcome (as in *W.S.*) or it may be ignored (as in *S.W.*).

There is little doubt that at times a proprioceptive element through convergence is an effective factor in controlled monocular vision of normal two-eyed subjects. Although not entirely to be trusted, *Ss* frequently gave evidence of this. The belief is sustained by the investigations on the histology of the extrinsic muscles of the eye. Several investigators, among them Huber,²⁸ Duke-Elder,²⁹ and Tozier and Sherrington,³⁰ have described the presence of an extraordinary number of special sensory end-organs in muscle preparations of the extrinsic muscles of the eye and of small medullated nerve fibres which end in these organs. Huber found 20 to 30 end-organs in one muscle in a number of his preparations. If this is true, we can assume a definite proprioceptive innervation from these striated muscles. Irvine and Ludvigh³¹ contradict the claim in two recent papers, mainly on the anatomical ground that these sensory end-organs are unlike proprioceptive organs in other striated muscles. If the proprioceptive nature of this innervation is accepted, then it is probable that some part of the complex experience of 'nearer' of these *Ss* as distinguished from 'farther' was affected by it.

There must be, however, some primary cue upon which this convergence depends, since it is convergence of the unobserving eye. This primary cue may come from accommodation on account of the normally close relation which has been built up between the two. As is well known, however, this relation is easily severed. The cue may come from retinal factors which give indication of the clearing up of the retinal image as a result of satisfactory adjustment of the lens. We know when the image is relatively clear; we do not know, however, when the lens is satisfactorily adjusted except by the end-effect, the clear image.

The crystalline lens is a mean by which light rays from different distances are refracted in the interests of clear vision. The ciliary muscle, according to the Helmholtz theory,³² by contraction so releases the lens that its anterior surface assumes the proper curvature for light-rays at a particular distance to be focused on the retina. For several decades the ciliary muscle has been assumed by many as supplying finely graded afferent impulses which play in from graded contractions of the muscle, attendant upon more or less passive dioptric changes in the lens. These finely graded afferent impulses are assumed to be the basis of discrimination of the relative distance involved, particularly in monocular depth.

²⁸ G. C. Huber, Sensory nerve terminations in the tendons of the extrinsic eye-muscles of cat, *J. Comp. Neur.*, 10, 1900, 152.

²⁹ W. S. Duke-Elder, New observations on the physiology of the extra-ocular muscles, *Trans. Ophth. Soc. Unit. King*, 50, 1930, 181.

³⁰ F. M. Tozier and C. S. Sherrington, Receptors and afferents of the 3rd, 4th, and 6th cranial nerves, *Proc. Roy. Soc.*, 82, 1910, 450.

³¹ S. R. Irvine, Histology of the extra-ocular muscles, *Arch. Ophth.*, 15, 1936, 847; S. R. Irvine and E. J. Ludvigh, Is proprioceptive sense concerned in vision? *Arch. Ophth.*, 15, 1936, 1037.

³² As far as contraction of ciliary muscle is concerned, the case is no different in the alternate theory of accommodation which involves the vitreous humor. W. H. Luedde, summarizes the case for the latter in The mechanism of accommodation, *Arch. Ophth.*, 7, 1932, and *Amer. J. Ophth.*, 19, 1936, 245-247.

That the character of the innervation for the ciliary muscle is clear is assumed in these discussions of accommodation. This assumption is debatable. Where the ciliary muscle is present in mammals it is smooth or 'involuntary' muscle. Birds and reptiles alone have striate musculature in ciliary and iris muscles. Smooth muscle is much slower and more feeble in contraction than skeletal muscle and in its fundamental properties shows well-marked differences. Ciliary muscle is no exception to this. The same sorts of efferent endings of the parasympathetic were found by Boeke³³ in ciliary muscle as in muscle of stomach and intestines. Moreover, this ending was strikingly similar to that ending of the sympathetic upon striate muscle-fibers which Boeke believes is responsible for tonus of skeletal muscle. Agababow³⁴ described nerve fibers in the ciliary body of albino cats, some of which seemed to him to be of a sensory nature. Boeke³⁵ confirmed this observation in his preparations of the human eye.

The presence of afferent ends is to be expected in smooth muscle. Their function, however, is much less clear than in striated muscles. It cannot be called kinaesthetic, and either proprioceptive or interoceptive classification is ambiguous and misleading. At any rate we have no evidence for any 'voluntary' innervation in this smooth muscle. Innervation occurs in connection with some other element, *i.e.* with convergence or with retinal factors or with both. From the evidence in this monocular experiment it is probable that here at least retinal factors formed the initiating stimulus, *i.e.* the diffusion circles (clarity and size) of the image. Such stimuli evidently continued until the image was clear, but the relations involved in the cessation of such stimuli from the retina are uncertain if not unknown.

It is doubtful, therefore, whether the activity of this smooth muscle plays the definite rôle which has been assigned to it in discussions of monocular depth-perception. It would be equally logical to expect discrimination of distance to be based upon impulses from the smooth muscles in the iris in the accommodation reflex which accompanies changes in accommodation and therefore changes in distances. This has never been proposed, at least not to the writer's knowledge.

(5) Finally, therefore, Ss who learned to discriminate distance did so on the basis of comparative blur in the retinal image with occasional reënförment or contradiction from a more or less involuntary comparison of the experience with the complex pattern of such discrimination under the usual circumstances. Convergence of the unobserving eye and movements of the eye-lids probably had some part in that reinforcement or contradiction. There is no indication of what is often called the immediate and familiar sensation of accommodation. Marked individual differences in behavior and in the graphic records show, moreover, that the discrimination, simple as it seemed to be, was a function of the total individual and not of this or that muscular sensation.

³³ J. Boeke, Die doppelte (motorische und sympathische) efferente Innervation der quergestreiften Muskelfasern, *Anat. Anz.*, 44, 1913, 349.

³⁴ A. Agababow, Die Innervation des Ciliarkörpers, *Anat. Anz.* 8, 1893, 555.

³⁵ J. Boeke, On the termination of the efferent nerves in plain muscle-cells, and its bearing on the sympathetic (accessory) innervation of striated muscle-fibre, *Proc. Roy. Acad. Amsterdam*, 17, 1915, 982.

